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“ *Ans.* There was not any sound heard: it was
“ supposed to be at too great a distance.”

These are all the accounts which I received, at different times, upon this subject. In another paper I shall offer to the Society some remarks, that have occurred to me, upon a careful review of the whole materials.

XXVII. *Some Remarks upon the several Accounts of the fiery Meteor (which appeared on Sunday the 26th of November, 1758), and upon other such Bodies; by John Pringle, M. D. F. R. S.*

Read Dec. 20, 1759. **I**N my last paper, I laid before the Society all the accounts I had received of the meteor, of the 26th of November, 1758; which I could not reduce into a narrower compass, without the hazard of omitting such observations, as might be judged necessary for ascertaining some of the principal circumstances; or without passing over those facts, which, however immaterial they may appear at present, might afterwards afford some light in explaining the nature of these *phænomena*. I have even inserted some particulars contradictory to others, that I imagined more likely to be true, as I myself might be deceived, and as, by preserving the several relations intire, I thereby did most justice to the observers. The deception of the senses, upon the appearance of unusual objects, the short continuance of the meteors, and

the surprize occasioned by them, at a late hour, among people unaccustomed to think on these subjects, will sufficiently account, not only for the variety, but the contradictions, in the several observations.

I come next to the result of all the relations, with regard to the true path, the velocity, figure, magnitude, and other circumstances relating to this body ; which having deduced in the best manner I could from the preceding materials, I shall now submit to the judgment of the Society. But as my last paper on this subject was of so great a length, I shall not farther insist on it now, than barely to mention the conclusions I have made, without losing time in recapitulating the several steps that led me to them *.

First then, as to the path. This meteor seems to have been vertical at Cambridge, or nearly so, and to have taken fire about the zenith of that place ; or at least to have appeared first there in a state of ignition. From thence it proceeded directly, almost N. W. by N. over several counties in England, over the Solway frith (which it crossed between Carlisle and the town of Dumfries) ; and in Scotland over the shires of Dumfries and Lanerk : but soon after its becoming vertical to the last, viz. a few miles to the southward of Douglas (or perhaps nearer to the borders of Lanerk and the shire of Air, about 10 or 12 miles to the eastward of Auchenleck), part of the tail seemed to break off, and to disperse in bright sparks of fire ; whilst the head, into which the remainder of the tail was instantly collected, moved on in the same

* Some of the most material are subjoined in notes to the former paper, since it was first presented to the Society.

direction,

direction, till coming over Fort William, in the shire of Inverness, after a course of about 400 miles, it there suddenly disappeared.

But, notwithstanding the extinction of the meteor at this place, it seems still to have proceeded northwards ; since it was seen again, in a luminous state, in a globular form, but without a tail, about the 58° of latitude, on the western coast of the shire of Ross, almost vertical to the observer ; moving then to the southward of the east ; that is, in a direction almost contrary to the first : and in this last course, of which we know not the end, it possibly might have gone a great way to the eastward.

During the first part of its progress, viz. from Cambridge to Fort William, it went obliquely downwards, in such a manner, that, by computation, it must have been from about 90 to 100 miles high at the first of these places, and between 26 and 32 miles at the last. But at what height it was afterwards seen in the shire of Ross, is not to be determined, since one observation only was transmitted to me from that country. As Dr. Mackenzie observed it nearly over his head, and yet of a smaller size than the full moon when some hours above the horizon, I should suspect, that, after its descent to Fort William, it had re-ascended at the time he saw it ; because he probably would have described it as a larger body, if then not higher than when it first disappeared.

This dipping and rising, in the course of a meteor, is not more extraordinary than its lateral deviation from a straight line, as I have elsewhere shewn*. Such

* See the note subjoined to Obs. XXVII.

are the facts, tho', perhaps, it may be hard to account for them from the common principles of motion. What seemed most likely to be the cause of the vertical change of its direction, was its rapid descent towards the earth, till it came so low as that the resistance of the air might act upon it; in which case, the lower part of the body meeting with denser air than the upper, it would be so reflected from that medium as to ascend; for tho', at the height of 20 or 30 miles, the air is extremely rarified, yet, in this instance, the immense velocity of the impingent body will make a less resisting medium produce a greater effect. The meteor, therefore, might be reflected by the air, in the same manner as a cannon ball by water, when it strikes it in a very oblique direction.

If this reasoning can be admitted, we may account for the lateral variation of its path, in the manner following. Although it should seem, that, during the first part of the course, the wind was at S. E. yet, when the meteor advanced towards the north of Scotland, it there probably met with a westerly wind; from this circumstance in Dr. Mackenzie's letter, *of the sea being that night remarkably louder than at any other time during a whole month he had been at Flowerdale*: for that place lies on the western ocean, in the shire of Ross; and there, I imagine, the sea could not produce that stormy noise, unless the wind had blown from some point between the west and the north. If therefore the current of the air obliquely opposed the motion of the meteor, that body would be gradually reflected into a new direction; from which, by another current, it might have been turned a second or a third time, until it fell at last into that path

path described by the doctor. From this account it will follow, that the lower a meteor descends, the more irregular its course will be; as it will then meet with the most resistance, both from the density and the currents of the air.

In regard to the velocity, it seems almost incredible; as we have sufficient *data* for computing it at the rate of 30 miles in a second. But if we allow, that it only moved through half the space in that time, we shall find the progression of this body to have been above 100 times swifter than the mean celerity of a cannon ball, and nearly equal to that of the earth in its orbit round the sun.

As to its real size, we cannot pretend to determine that point with any precision; since its dazzling brightness would occasion some deception, and the apparent magnitude has been so differently represented by the observers. If the meteor, when nearest to Dublin, appeared to Mr. Garret equal to the full moon, then we shall find, that its true diameter was about two miles; and if the farmer at Ancram saw this body, whilst it was vertical at Cambridge, of a size equal to the crown of his cap, or to about half that of the full moon, we cannot allow less than a mile for the real axis. Upon estimating from the observations made at St. Andrews and Dunfermline, the diameter was at least half a mile, and perhaps much greater. However, as the imagination is so apt to enlarge such objects, we shall put the size of the globe at the smallest, and reckon it only about a mile and a half round.

The body must surely have been of a considerable bulk to have yielded such a light, as that, when in the
zenith:

zenith of Cambridge, the farmer at Ancram (at the distance of above 260 miles) should, upon entering his threshold, see the whole side of his house illuminated by it; and, to use his own expression, *with a brightness as of sun-shine*. And indeed the greatness of the light is every-where taken notice of, even at those places where the atmosphere was so thick as to hide the tail, nay, the whole meteor, as at Auchenneck, where it was nearly vertical.

As for the tail, it was a stream of light several miles in length; for this was no deception, like what we suppose the train of a shooting star to be, but was either a real flame, or, what is more probable, it consisted partly of flame, but mostly of smaller masses of fire (which the observers call *sparks*, when falling out of the lucid tract), and of vapours or fuliginous particles, not heated red-hot, but illuminated by the parts actually burning. Perhaps these vapours were the chief part of the composition, and which will account for its light being so much fainter than that of the head; since in some places where the air was less clear, or the distance greater, we find the whole meteor described either as a round ball, or a spheroid (with the largest axis in the direction of its motion), but without a tail. In this last case, viz. that of the oval form, it is probable that, besides the head, the beginning of the tail was also visible, as consisting of flame, and therefore brighter than the rest; and that both together appeared oblong to those observers. But such as were nearest, and had a clear atmosphere, saw the tail of a considerable length; that is, the flame, the sparks and the illuminated vapour, in a train behind the head, as being lighter,

and therefore meeting with more resistance from the air ; in the same manner as the flame, the sparks, and smোক, of a torch are seen to follow it. All this is plain ; but, in regard to that separation of the third part of the tail from the rest, a circumstance clearly described by the farmer at Ancram, and seemingly confirmed by other observations, there may be some difficulty. Perhaps at this period, upon a greater explosion in the ball, most of the combustible matter was thrown out at once, which falling behind, occasioned that appearance of the breaking off a part of the tail, whilst, for want of fuel, the remainder vanished, or, as the observer expresses it, *was collected into the head*. This account is rendered more probable, by what is said of the emission of a greater light about this time, and by the loud report heard by the farmer five minutes after, and which, upon computing the distance, ought to have reached him much about that time, had it been occasioned by this extraordinary bursting and dispersion of the inflammable matter.

The hissing noise, taken notice of by some while the meteor passed them, was a deception of that kind, which frequently connects sound with motion ; and is the case of those who fancy they hear something, when they see the shootings of the *aurora borealis* ; I say a deception, because if the meteor, during its course, really made any noise, so great was the distance of that body, and so short its continuance, that this sound could not have been heard till some minutes after the return of darkness. But the final report, so frequently mentioned, not only heard by those who saw the light, but by others who knew

nothing of what had happened, was a real found, and immensely greater than any we are acquainted with. For, at the distance of 70 miles and upwards, it was compared to loud thunder, the report of heavy artillery, the fall of the gabel-end of the house the person was in, and to a musket fired off in the garret. If this noise was produced when the body threw out those masses of burning matter (by the observers called sparks of fire, the bursting of the tail, and delineated in *Fig. 5.* as balls of a smaller size in the train itself), we shall find, that at this time the meteor, by being more than 41 miles high, was in a region where the air is three thousand times rarer than on the surface of the earth; that is, about six times rarer than in a common exhausted receiver, where sonorous bodies are not heard, and even where gunpowder and the *pulvis fulminans* take fire, and are exploded, but without noise. Hence I would infer, that the separation of the elastic matter must have been performed with a velocity exceeding all imagination; as the intensity of sound so much depends on the resistance of the air, and as this elastic matter could fly off with so much celerity, as to find so great an opposition from so thin a medium*.

I should also conclude from the great report, that the substance of the meteor was of a firmer texture than what could arise from mere exhalations, whether formed into a sphere, and then burning; or dis-

* *M. Saluce* has lately shewn, by some curious experiments, that such substances as gunpowder and the *pulvis fulminans*, have a detonation in proportion to the rapidity with which the internal air is separated, and to the resistance of the external air. See *Miscell. Taurinens. Tom. I.*

posed into a kind of train, and consumed by a running fire : for sounds, as far as we know, are either produced by the quick and violent percussions of hard bodies upon the air ; or by the sudden expansion of an elastic fluid, after being condensed within some solid substance. The noise occasioned by the motion of electrical matter is, perhaps, the only exception ; but we have no reason to imagine, that this was at all concerned in the present case. There seems to be the more ground for believing this body was solid, at least that its surface was so, from finding, that, after the violent explosion, it still retained its form ; a circumstance that could hardly take place, if the meteor had consisted of nothing but vapours. We may therefore presume, that the burning matter found vent through a hard crust by certain apertures, which either might have been there invisible, or unobserved. All I can say in support of this conjecture is, that, by the Memoires of the Academy of Bologna, we find a meteor appeared in Italy in the year 1719, lower in the air than that we have now been treating of, and in which, it is pretended, four several chasms were distinguished, each emitting smoke *. To these arguments for the solidity of this body, we may add its extreme velocity, and the intensity of the light ; which are likewise circumstances more conformable to a heavy and solid substance, than to one formed of exhalations only.

Upon the whole, I believe it will appear, that these relations are not favourable to the prevailing

* *Apparebant in eo hiatus seu voragine quatuor fumum exhalantes. Instit. & Acad. Bonon. Tom. I. p. 285.*

hypothesis about the formation of such bodies, which makes them to consist of certain sulphureous vapours arising from the earth: for, besides what has been urged above, Dr. Halley has shewn, " That, at the " height of 41 miles, the air is so rarified as to take " up 3000 times the space it occupies on the surface " of the earth; and that, at 53 miles high, it would " be expanded above 30000 times: but thinks it is " probable, that the utmost power of its spring cannot " exert itself to so great an extension; and that no " part of the atmosphere reaches above 45 miles *." This being the case, how can we suppose any such vapours to rise to the height of 90 or 100 miles, where the air must be so many millions of times rarer than what we breathe? Again, we find that all vapours, in their ascent, mutually repel one other; so little do they seem disposed to run together into globes of such a size. But Dr. Halley observes (in treating of the great meteor of March 1719), " That " tho' the aqueous, and most of the other vapours, " are soon condensed by cold; and wanting a certain " degree of specific gravity in the air to buoy them " up, ascend but to a small height;" yet he adds, in favour of the opinion of these bodies being formed of exhalations, " That the inflammable sulphureous " vapours, by an innate levity, have a sort of *vis* " *centrifuga*, and not only have no need of the air " to support them, but, being agitated by heat, will " ascend in *vacuo Boileano*." From hence he infers, " That the sulphureous vapours, disengaged from all " other particles, may rise far above the reputed

* Phil. Trans. N^o 181. p. 104. Abridg. Vol. II. ch. 1. § 9.

" limits

“ limits of the atmosphere; and contracting them-
 “ selves into a narrower compass, by that principle
 “ of nature that congregates the *homogenea*, may lie
 “ like a train of gunpowder in the æther, till catch-
 “ ing fire by some internal ferment, the flame may
 “ be communicated to its continued parts, and so
 “ run on like a train fired,” &c *. But this *hypo-*
thesis of that excellent naturalist seems liable to the
 following difficulties. 1. What are the experiments
 which shew, that inflammable vapours have any sort
 of a *vis centrifuga*; or will rise either in an absolute
 vacuum, or in a medium where the meteors are often
 found, and where, according to the doctor, the air
 must be five hundred times rarer than in a common
 exhausted receiver †? 2. Altho’ sulphureous vapours
 may ascend from the earth, upon being agitated by
 heat, will not that volatility soon cease, by the cold-
 ness of the air, long before they can arrive at the up-
 per regions? 3. Does not all matter, capable of
 taking flame, suppose a proportion of an acid salt,
 adherent to the phlogistic principle, and consequently
 some gravitation? 4. And will not the weight be
 increased by the large quantity of condensed air, or
 some other elastic fluid combined with the inflam-
 mable vapours, which is the cause of the explosion
 and report? 5. Are we not led to this notion about
 the innate levity and *vis centrifuga* of igneous matter,
 from finding, that heat has a greater tendency upwards

* Phil. Transf. N^o 360. p. 978. Abr. Vol. IV. Par. II. Ch. 1.
 § 28.

† His expression is, *above three hundred thousand times rarer than
 what we breathe.*

than downwards? but is not this owing to that portion of the air, which, receiving the heat, and being thereby dilated, becomes specifically lighter than the rest, and therefore rises in the colder and more condensed atmosphere? 6. Suppose attraction to take place, should not those vapours, by that law, assume a globular form, and not, like a train, contract in breadth, and extend in length? 7. What is to set this train on fire; since a ferment, according to the chemists, implies a mixture of heterogeneous parts, contrary to the doctor's *hypothesis*? 8. How comes this train to be drawn out in so straight a line, and of so equal a breadth, insomuch that, in the path of the late meteor, for about 400 miles, there was no sensible deviation, nor any change in the magnitude of the luminous appearance, but what might be accounted for from its greater or less distance from the observer, or its being seen through a clearer or an obscurer atmosphere?

Some have been of opinion, that these fiery meteors are only a kind of lightning, at greater heights than common; forming their notion upon the velocity of those balls of fire, and upon the sound accompanying them, so much resembling that of thunder. But this *hypothesis* having gained no credit, I need not employ time in refuting it; and the less now, as the nature of lightning is so much better understood than when this theory was first published. I shall only observe, that before the matter of lightning was discovered to be of the electrical kind, it was natural to suppose it to be formed of the sulphureous vapours arising from the earth; and if the earth was found proper for producing such exhalations, of
course

course it was judged capable of furnishing materials for all the lucid *phænomena* in the æthereal regions. Thus, not an hundred years ago, the comets themselves were accounted for on no better principle; and therefore we are the less to wonder if these meteors have been hitherto almost constantly referred, even by the best naturalists, to the same origin.

Of all the *hypotheses* that have come to my knowledge on this subject, a hint of Dr. Halley's (in a paper presented to the Society several years before the above-mentioned), seems best to agree with the late meteor; viz. that such bodies may be formed independent of any vapours from the earth*. But, with regard to the rest of his conjecture, that the one he described, "might be a collection of matter formed in
 " the æther by some fortuitous concurrence of atoms;
 " and that the earth had met with it newly formed,
 " and before it had conceived any great *impetus* of
 " descent towards the sun," I say, with regard to these circumstances, they are not at all supported by the present case: for, if we consider the immense velocity with which the late meteor moved northwards (and in judging of which we could not be deceived by either the diurnal or annual motion of the earth), we must be convinced that the earth did not meet with it before it had conceived a considerable *impetus* of descent, either towards the sun, or some other attracting power. And altho' there was likewise a motion of this body towards the earth, as appeared by the dipping of its path, yet it did not come quite down,

* Phil. Transf. N^o 341. p. 159. Abr. Vol. IV, Par. II, Ch. 1. § 25.

as might have been expected, had gravitation to the earth been the only principle concerned; but it descended no lower than to a certain depth in the atmosphere, after which it seems to have risen again, and made a considerable progress in the higher regions; contrary to Dr. Halley's opinion about such meteors, which, he believed, actually came to the ground *. And here I will venture to affirm, that, after perusing all the accounts I could find of these *phænomena*, I have met with no well-vouched instance of such an event: nor is it to be imagined but that, considering the frequency of such appearances, if these meteors had really fallen, there must have been long ago so strong evidence of the fact, as to leave no room to doubt of it at present. Their descent, under the horizon, is sufficient to make the common observers believe they see them come to the ground, whilst an explosion, high up in the air, coming late to their ears, passes for the crashing noise of the fall. Not that I call in question the possibility of their touching the earth; which they are likely to do as often as they move perpendicularly towards it, and not in that oblique direction so often mentioned, and by which means, it should seem, they are constantly reflected by our atmosphere. All that I would conclude is, that hitherto we have had no certain proof of their fall; and it is to be hoped, that their motions, like those of the comets, have been so regulated at first by a governing Power, that we have nothing to apprehend from their aberration. Unless we should imagine, that the perpendicular descent and bursting of

* Vide loc. cit.

one of these bodies has given rise to the story of Phaëton; since no other event will, perhaps, so well account for not only the foundation of that fable, but for a prevailing opinion among the ancients, that, besides a large tract of country, even the heavens themselves, in appearance, had once suffered by a conflagration.

If it is then probable, that these balls of fire come from regions far beyond the reach of our vapours; if they approach often so near to the earth, and so seldom or never touch it; if they are moved with so much celerity, as in that respect to have the character of celestial bodies; if they are seen flying in all directions, and consequently have a motion of their own, independent of that of our globe; if they part with such quantities of an elastic fluid, a phlogistic matter, and probably an acid, surely we are not to consider them as indifferent to us, much less as fortuitous masses, or trains of terrestrial exhalations in the æthereal regions; but rather as bodies of a nobler origin, possibly revolving about some center, formed and regulated by the Creator for wise and beneficent purposes, even with regard to our atmosphere; which, during their combustion, they may supply with some subtile and salutary matter, or remove from it such parts as begin to be superfluous, or noxious to the inhabitants of the earth.

Since these sheets were printed off, Dr. Pringle received another letter from Sir Robert Pringle, acquainting him, he had not found that part of his intelligence hold good, of the ship-master's seeing the meteor (of the 26th of November 1758) in the Baltick; but, upon further inquiry, had learnt, that though this person had observed an appearance of the same kind in those seas, it was not on the same day.

Dr. Pringle thinks it likewise proper to inform the public, that, since his paper was printed, he has received two further accounts of the meteor, one from Mr. Pringle, sheriff-depute of the shire of Roxburgh, the other from Mr. Garret, of Island-Bridge, near Dublin. Mr. Pringle says, " that having, since his last letter, drawn a more accurate meridian, at the farmer's house at Ancram, he now finds, " that the shepherd's house (mentioned p. 236) bears nearly S. E. by S. from the farmer's dwelling house; that the hill with the trees, " over which the meteor seemed to break (laid down in *Fig. IV.* as at $92^{\circ} 30'$ W. of the meridian), really bore W. by N. from the " observer; and that the bearing of the luminous body, at the point " of its extinction, was nearly W. N. W. and not due N. W. as the " farmer at first imagined, who, upon this last survey, was convinced " of his mistake." Upon this communication, Dr. Pringle drew on two different maps, a W. N. W. line from Ancram to the path of the meteor, and found, that in Elphinston's map of Scotland, the intersection was a few miles to the eastward of Glasgow, but in Kitchen's map a little to the westward of it. In consequence of these last observations, the Doctor has fixed the point of extinction to the zenith of that city. But as Glasgow lies about 76 miles to the southward of Fort William, over which the meteor, in the preceding paper, was supposed to disappear, the first course of that body is reduced to about 324 miles in thirteen seconds of time, and its velocity therefore to about 25 miles in one second. Mr. Pringle has likewise acquainted the Doctor, " that the farmer, on seeing the engraving of the 4th " figure (of which Mr. Pringle had received a copy), had found fault " with the size of the head of the meteor, as being too large in proportion to the length of the whole; since, to his imagination, the " diameter of the head did not exceed 8 inches, and that the tail seemed " to be about 3 or 4 yards long."

Mr. Garret acquaints Dr. Pringle, " that, since his first letter to the " Doctor, Emanuel Miller and he, having communicated their observations to Mr. William Gibson, mathematician, at Dublin, that " gentleman had come to Island-Bridge, and having made a survey " with his instruments, found, that the greatest altitude of the meteor " was no more than 12 degrees."

ERRATA.

P. 240. *lin. penult.* for P Q R U, *the path of the tail broke off over the trees*, read Q R S U *the path.* E F *the tail broke off.*

In the plate, E F *the breaking of the tail*, should not have been represented as directly over the trees, but a little more to the southward, so as to bring the ball W (*viz.* the meteor at its extinction) perpendicularly over the said trees.